substracted to know at what time the said spot is to come into the middle of *Jupiter's* diske." In all of which Cassini only anticipated Mr. Marth by two hundred years or so. In connection with this it is noteworthy that in the year 1858 a remarkable dark spot appeared upon the face of *Jupiter*, in the same part of his disc. My first intention was to have copied a drawing of my own, made at this date, to illustrate this, but I find one by Mr. Lassell on p. 52 of vol. xix. of the *Monthly Notices*, in which the spot is depicted. Putting all this together, it would appear that a permanent source of disturbance exists in a very circumscribed region to the south of *Jupiter's* equator, and that in observing the red spot we are probably only watching the marking by which the first determination of *Jupiter's* period of rotation was ever made.

On Prof. G. W. Hill's Paper on Delaunay's Method.
By Edmund Neison.

It had been my intention to have passed over in silence Prof. Hill's paper in the November number of the *Monthly Notices*, entitled, "A Reply to Mr. Neison's Strictures on Delaunay's Method of Determining the Planetary Perturbations of the Moon;" but it has been pointed out to me that, as the question involved is one which cannot readily be followed by most astronomers, it is proper that I should make known my dissent from the superficial criticisms urged by Prof. Hill, based as they are on so much misconception, betraying as they do such imperfect acquaintance with the previous history of the question, and supported as they are by no evidence beyond strong unsupported assertion. The whole paper is unjustifiable, unless we accept as incontrovertible the point of view from which the whole is written—namely that, because I differ from Delaunay, I must have a confused, erroneous conception of his method, and must be entirely in the wrong! The very keynote to the tone of the paper is given in almost the opening paragraph, that "If we were obliged to admit the validity of *all* the statements in this article, an easy corollary from them would be that Lagrange's general method of the variation of arbitrary constants in the problems of mechanics was a blunder. Now, I think that no one acquainted with this method could, for a moment even, entertain such a proposition. Hence, we may conclude there is some flaw in the reasoning of this paper." As a simple assertion this is strong enough and, polemically, may be good, but if Prof. Hill undertakes to substantiate his assertion it will be *he* who will have made the blunder.

I do not wish even to seem wanting in courtesy to Prof. Hill, whose valuable contributions to the Lunar Theory have always commanded my warmest admiration; but I cannot admit that the objections raised by him in this paper are such as to call for any serious reply.

Perhaps there are two points which might be noticed.

1. Prof. Hill's justification of Delaunay is singularly enough the simple reiteration of the unquestionable error dealt with in my very paper, where I clearly showed that it was no justification of Delaunay to say that he neglected these quantities depending on m^4, m^5, m^6, m^7 . . . ., because they were beyond the order of approximation fixed by him, and seemed obviously to be very small, because in the face of Hansen's explicit statement that, according to his calculations, it was these terms and these only which yielded the large values found by him, Delaunay was bound to so fix the order of his approximations that these terms were included. For remember that Delaunay was verifying Hansen's results; he was bound to include all included by Hansen, and in interpreting Hansen's statements was bound to assign Hansen's meaning to Hansen's words; so it is useless to try to evade the difficulty, as Prof. Hill does on p. 2, by assigning to Hansen's words a new and forced meaning that was never employed by Hansen or any of his contemporaries.

2. Prof. Hill points out how amusing it would be if it should "turn out that the set of values withheld from publication by Hansen were identical with those of Delaunay." Amusing! Very! in the face of Hansen's letter to Leverrier shortly before his death, in which he again expresses his complete disagreement with Delaunay's results, and his conviction from his calculations that, despite Delaunay, both the terms in question had sensible coefficients.

The question at issue may be stated thus:

By the ordinary direct methods of dealing with this problem, methods whose soundness, simplicity, and adequacy is unquestionable, there are obtained in the value of these coefficients certain quantities of the order m^6, m^7 . . . . depending on the higher powers of the disturbing force, which give rise to sensible values to these terms of long period. By the use of Delaunay's complex method neither he nor his followers have been able to obtain these terms. They argue—We do not obtain them, therefore they cannot exist. Hence, you ought not to get them by the ordinary direct methods, consequently those you do get must really disappear in some unexplained manner. The reply is—No; the onus of proving this disappearance lies with you; if you cannot derive them from your special method, the presumption is that you are not properly using your special method, so it is yielding you imperfect results.

Having now explicitly expressed my dissent from Prof. Hill's criticisms, I shall defer all further remarks to my Memoir on this subject, when I will show still more explicitly than I have yet done how Delaunay's application of his method to the computation of these terms is vitiated by an implicit assumption analogous in principle to that which would be made by assuming

$$f(\mathrm{R} \cdot \mathrm{R}' + \mathrm{R}'') = f(\mathrm{R} \cdot \mathrm{R}') + f(\mathrm{R} \cdot \mathrm{R}'').$$

Natal Observatory:
    1887, *April* 11.

Note on the Performance of the Westminster Clock.
By Thomas Buckney.

The reputation of the great clock of the Houses of Parliament is so well established that some adequate reason must be given to justify any further reference to its performance here. This will, I think, be furnished by an inspection of its error from Greenwich time on each day of the period under review, viz., from March 29 last until June 9. The clock, as is well understood, is not allowed to run for an indefinite time with an accumulated error like an astronomical clock, but is kept as close to Greenwich time as possible, the necessary correction being made as soon as the error reaches 2 seconds either fast or slow. The pendulum is never stopped for this purpose, but alterations of rate are effected by the addition or removal of small weights in such a way as practically to shorten or lengthen the pendulum, whilst errors of 4 seconds or more are corrected by stopping the train of wheels or allowing them to run on for a few seconds as may be necessary. Four seconds is, however, the smallest alteration that can be made in this way. Now the clock, which, in common with most others in the country, was stopped by the snowstorm of Dec. 26 last (the snow having blocked the path of the hands), had since been going very well, requiring, however, small corrections from time to time. The last of these was made on March 29, and since that day no alteration or correction whatever has been made.

The following table shows the daily error of the clock since that time, and the figures, which have been kindly furnished by the Astronomer Royal from the records of the Royal Observatory, may be taken as authentic. It should be mentioned that the clock automatically reports itself by electric current to the Royal Observatory twice daily:

1887.		secs.	1887.		secs.	1887.		secs.	1887.		secs.
March	29	−0·0	April	9	−2·0	April	20	−1·0	May	1	−1·0
	30	−0·0		10	−*		21	−2·0		2	−2·0
	31	−1·0		11	−1·0		22	−2·0		3	−1·0
April	1	−1·0		12	−1·0		23	−1·0		4	−1·0
	2	−1·0		13	−1·0		24	−1·5		5	−1·0
	3	−3·0		14	−1·0		25	−2·0		6	−1·0
	4	−2·0		15	−1·0		26	−1·0		7	−1·0
	5	−2·0		16	−1·0		27	−1·5		8	−1·0
	6	−2·0		17	−1·0		28	−2·0		9	−1·0
	7	−2·0		18	−1·0		29	−1·5		10	−1·0
	8	−*		19	−1·0		30	−1·5		11	−2·0

* No observation taken on these days.